AN EXPERIMENTAL STUDY OF FUNCTIONAL ANESTHESIAS AS INDUCED BY SUGGESTION IN HYPNOSIS

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1: *Report of the Experiments*[1]

THE experiments, to be reported, were carried out in the new Department of Abnormal Psychology at Harvard University, during a large part of the college year 1926–27 and during the autumn, 1927. Valuable assistance during the experimentation was given by Mrs. H. Lundholm, and by two students of psychology, Miss M. B. Mitchell of Radcliffe and Mr. L. R. Weiss of Harvard. The subjects, all men and volunteers, were not, with the exception of one, students of psychology, and were, as we think, nonbiased by any theory concerning the nature of the experiments.

The main purpose of the experiments has been to try to investigate, by means of a conditioned response method, what is called mental dissociation, whatever the latter may mean. More specifically the problem has been to study artificially induced anesthesias by investigating their influence both on the conditioning of responses and on responses already conditioned.

The very first experiments were determined by a notion that we conceived of as being possible, namely, that a response may be conditioned by medium of a *co-conscious* or *subconscious* nucleus of awareness, which the subject thus during the experimentation is not consciously aware of. Accordingly we began by trying to condition a response by a stimulation for which the subjects had an artificially induced anesthesia. We had devised, a priori, a whole series of further experiments that were going to follow this, but as our devise was based on certain expectations of what was going to happen, and as the happenings did not follow our expectations, we were already in the very beginning sidetracked from our plans and from a strictly laid out method of procedure, and after this the work has been more like the exploring of a new land, and the route has been full of excitements and surprises.

[1] This report was read at the Psychological Clinic of Harvard University, on January 13, 1928

However, we have tried and, as we think, partly succeeded in tracing some psychological principles connecting the phenomena, as they have been directly given by the experiments.

We have been working with four men subjects, all of whom we could easily bring into a deep state of hypnosis, and all of whom showed complete post-hypnotic amnesia for any suggestion given during the hypnotic sleep. The results from two of the subjects have all through been similar, and, hence, we need in the coming report only to follow one of these. The phenomena connected with the third and fourth subjects, on the other hand, present deviations that will be treated separately. It should be noticed that the different moves of the experiments to be described, all cover large numbers of series of trials, generally undertaken on several consecutive days. A series of trials always contained 100 stimulations, out of which 80 were stimulations with both adequate and nonadequate stimuli, and 20 (irregularly spread through the series) stimulations with only the nonadequate stimulus. In the first part of the experiments we used electrical shock as the adequate stimulus for a withdrawal response of the right middle finger from the electrical contact, a sound-click, produced by the ordinary telegraphic sounder as nonadequate stimulus for the same response. In the latter part we repeated certain features of the experiments, using electric shock and the flashing up of a small electrical lamp as stimuli, and added a few new features with this laboratory device.

In accordance with our plan, we started the experiments with Mr. V. by trying to build up a conditioned response to a stimulation for which the subject during the conditioning had an artificially induced anesthesia. The experiments were carried out with the subject in a post-hypnotic, fully waking condition, but in which he was deaf for the sound-click, the deafness being due to preceding suggestion during hypnotic sleep. The result of this attempt was entirely negative. Our preconceived notion that a conditioned response possibly might be built up by medium of a coconscious fact of awareness proved to be wrong. After 12 series of experimentation, containing altogether not less than 1200 trials, there was not the vaguest indication of a tendency to withdraw the finger from the electrical contact when the click was sounded without the shock. In the normal individual this withdrawal becomes a fairly well established habit after only 50 trials or less. In fact, the conditioned response we have chosen to work with is one of the most easily established of all.

Our next move was now to build up the conditioned response in fully normal condition, when the subject was clearly aware of both the adequate and the nonadequate stimulations. The first series of trials seemed to indicate a tendency of establishment of the conditioned response but as we continued, this tendency entirely disappeared and we arrived at a point when there was not a single withdrawal on click without shock. This was rather surprising and we asked ourselves how it might have come about. The answer that first occurred to us was that perhaps the subject, for some or other reason, might have built up an attitude of consciously wishing to resist the impulse of withdrawal when the sound alone was given. We made a few series of experiments on normal control subjects, showing that if they are instructed to try to resist or inhibit the withdrawal, except when they feel the shock, they may partly succeed in doing so, however not nearly as well as our subject V. While in this attitude the control subjects were noticed to show a facial expression of utmost attention to the performance of the experiment all through the series, while Mr. V. showed a rather blank and noninterested facial expression all the time. A further line of reasoning was this: If our subject V. consciously attempts to inhibit impulses to withdraw his finger when he is stimulated only by the sound, distraction of his attention during the performance ought to interfere with this inhibition. Consequently, we carried him through two series of experiments, during which his attention was distracted by mathematical calculation. We had him, while he was doing the reactions, subtract successively 7 from 100, etc., and later add, subtract, multiply and divide numbers read to him by one of the assistant experimentors. Even under such conditions there was not one single withdrawal except when the shock was given. Still another trick was tried to see if we could interfere with the inhibition. We instructed the subject to pay attention to the sound, not to his finger, during the experiment. We suspected that the subject previously perhaps had paid strong attention to the finger, and that this might have resulted in the successful inhibition. As before there was no withdrawal except on the shock.

By and by the thought came to our mind that this inhibition—we could not possibly rid ourselves from the view that there had been some kind of inhibition at work—perhaps was not at all consciously willed, but may have been due to some or other coconscious activity, and we could theoretically conceive of the possibility that such an action tendency perhaps had been

strongly established during the first phase of the experiment, when the subject was in the post-hypnotic condition and deaf for the nonadequate stimulus. Although in these experiments the only thing suggested to the subject had been post-hypnotic anesthesia for a certain sound, it may be that this suggestion had associated with it a positive tendency of inhibition, relative to withdrawal on the stimulation that was not consciously perceived. Further more this inhibitory tendency may have been so strongly established during the experiments, referred to, that it outlasted the conditions of the second phase of experimentation, still influencing the reactions of the subject in apparently normal condition. If this working hypothesis were true it seemed to us that a contra-suggestion, given to the subject in hypnosis, ought to do away with the inhibition. Accordingly we hypnotized the subject and gave him the following suggestion: "When you wake up you will not resist any impulse to withdraw your finger from the electrical contact, not even the vaguest impulses." Then we woke him up and tried him again. Just as before, not a single withdrawal on sound without shock. The inhibitory tendency, whatever it may be, was equally effective after the contra-suggestion.

We were puzzled and quite discouraged, and tried all kinds of reasonings to explain our failure to break down the inhibitory impulse. Finally we made one more attempt, this determined by the following consideration: perhaps our contra-suggestion had not reached the hypnosis integrate of personality that was inclusive of the inhibitory tendency. We hypnotized the subject once more and this time we investigated the content of his mind during the sleep by questioning him concerning the experiments. And now it was shown, that first we obtained an integrate that remembered having heard the click, but that was neither aware of having felt any impulse of withdrawal when the click was heard without the shock given, nor of having ever inhibited or resisted any impulse to withdraw the finger from the contact during the experiment. Only by bringing the subject repeatedly into deeper and deeper sleep did we finally reach another integrate, that admitted both having heard the click, having felt an impulse to withdraw on click without shock and having resisted and inhibited that impulse. To this all inclusive integrate we repeated the contra-suggestion. And now we got, when we again continued the experiment through two series of trials, 26 withdrawal responses to sound without shock, out of 40 stimulations by sound without shock, showing that this time the contra-suggestion had been effective. At

the following session of experimentation, two days later, the effect of the contra-suggestion was still there. We got 70 withdrawals on sound without shock out of 200 stimulation with sound without shock. Besides that we get a new kind of incomplete withdrawal-responses on sound without shock, that by the observer was described as follows: "This response is not a withdrawal. The finger never leaves the contact, but there is a definite jerk of the hand in these instances." Seventy-four such responses occurred in addition to the 70 completely positive responses in the 200 trials with sound without shock. We had already at the previous session ventured to interpret the existence of both withdrawal responses and absence of such when the sound alone was given as indicative of a co-conscious conflict or battle between an inhibitory tendency and another tendency set up by the contra-suggestion, a battle in which sometimes one, sometimes the other tendency was the victor. Now we felt that this new type of half responses might be expressive of the same conflict. When we continued the experiments again two days later the effect of the contra-suggestion was diminished and seemed to gradually fade away. It was however still manifested in 35 positive responses and 35 half responses out of 120 trials with nonadequate stimulus alone.

The next move of the experiment was determined by the following consideration: if the performance we had seen during the last three days of experimentation was due to a co-conscious conflict, we ought to be able to solve this conflict by strengthening in turn one or the other of the conflicting tendencies. The solving of the conflict in favor of the tendency of the contra-suggestion would theoretically be brought about by repeating forcefully the contra-suggestion in hypnosis, the solution of the conflict in favor of the inhibitory tendency would be brought about by letting the subject again go through a number of series of trials in a post-hypnotic condition with deafness for the non-adequate stimulus. As you remember, we had assumed that this inhibitory tendency originally had been built up during the first phase of the experiment, when the subject in post-hypnotic condition was deaf to the sound-click.

In accordance with this working hypothesis we hypnotized the subject and gave again the contra-suggestion. The experiments then carried out showed withdrawal response every single time the nonadequate stimulus was given alone. Then we hypnotized the subject again giving him the suggestion that when he woke up he would not hear any other sound than the hypnotizer's voice.

We woke him up, assured ourselves that in the post-hypnotic condition he really was deaf for the sound-click, and continued the experiment. Now the withdrawal responses on the nonadequate stimulus alone was entirely gone. Not a single one occurred. Finally we brought the subject back in normal condition, when he again heard the sound, and now we got back the typical conflict series. There were 12 withdrawal responses, 5 half withdrawals, and 3 negative responses in a series where altogether 20 stimulations with sound without shock occurred. Our last experiments had thus apparently shown that we could switch strength to any-one of the conflicting tendencies by repeating the contra-suggestion on the one hand and by carrying on experiments in post-hypnotic condition with anesthesia for the nonadequate stimulation, on the other. We succeeded fully in solving the conflict by thus favoring the tendency of the contra-suggestion, but did not succeed fully in doing the same by favoring the inhibitory tendency.

When we had conducted the experiments with sound and shock so far we decided to try to repeat them with light and shock. We brought the subject into a post-hypnotic condition in which he was blind for the electrical lamp, the lighting up of which was the nonadequate stimulus in these series. As before we failed to build up the conditioned response. After twenty-eight series containing not less than 2800 trials, there was not a single withdrawal from the contact when the light-flash alone was presented. We then continued to experiment with the subject in normal condition when he was fully aware of both the light and the shock. Contrary to what we obtained working with sound and shock, we now succeeded in establishing the conditioned response. Beginning with the fifth series and all through 20 following, there was withdrawal every time the nonadequate stimulus alone had been given. After 25 series of experiments in normal condition we again brought our subject in a post-hypnotic condition where he was blind for the lamp. Immediately every withdrawal response to light without shock completely disappeared, this in spite of the fact that we had established a very strong habit of withdrawal on light without shock during the experiments in normal condition.

Here we had to interrupt the experiments with subject V. as the term had come to its end.

The experiments with the second subject did in general run in the same way as the ones with Mr. V. We could, however, only use sound and shock as stimuli for the reason that we were unable to make this subject blind for the lamp.

The experiments with the third subject Mr. N. began in the same way as before. We tried to establish a conditioned response in post-hypnotic condition when the subject was deaf for the non-adequate sound stimulus. As before we failed to build up the conditioned response. We then continued with experiments in normal condition, and now we could establish the conditioned response easily. In this subject, there was no outlasting of an inhibitory tendency from the first phase of experimentation. We made a number of series alternatingly in post-hypnotic condition and in normal condition. Whenever there was deafness for sound all withdrawal responses on sound without shock disappeared. Whenever the subject perceived the sound there was withdrawal on sound without shock.

After this we tried an entirely new experiment. We wanted to see if we could condition a response by a positive hallucination, that is by a fact of awareness that is not due to real perceptual stimulation but due to suggestion only. The following method of investigation was used. We hypnotized the subject and gave him the suggestion that when he woke up he would see a small electrical lamp light up every time he got an electrical shock. This lamp was placed somewhat temporal in front of the subject. Our plan was this: if we could make the suggestion effective, every now and then, to really light up the lamp without giving the shock, and see if the subject would withdraw his finger on real light stimulation alone without shock. During the conditioning, of course, we would not light the lamp, simultaneously with the shock, or in other words, the conditioning stimulus would be merely a positive hallucination of the flashing up of the lamp. We succeeded in making the suggestion effective, that is the subject claimed in a post-hypnotic condition that every time he felt the shock he saw the lamp flash. The series of experiments we now carried him through were of the following kind. Each series contained as before 100 trials. Of these trials 80 consisted of stimulation with shock alone accompanied by hallucinatory light, 20 of stimulation with actual light alone without shock. On all the stimulations with light alone, except twice, in the first series of 100 trials, the subject withdrew his finger from the electrical contact. We repeated the experiment on several consecutive days, and found there was always withdrawal when the light was really lit up *without* the shock being given. We had consequently established a conditioned response to actual light on the basis of a positive hallucination.

It is interesting to compare these experiments with the previous ones. In our first experiments the negative hallucination or the anesthesia seems to associate itself, by some principle, with a negative behavior tendency (inhibition of withdrawal to the non-perceived stimulus) while in the last experiments the positive hallucination associates itself with a positive behavior tendency (withdrawal when the light is really lit up). It may not from one point of view be correct to speak of the former behavior tendency as negative, as the inhibition of an impulse to withdraw undoubtedly is also a positive tendency, and probably rather strong as such. When we speak of it as negative we only mean that it is so from the point of view of its overt expression (absence of withdrawal from the contact). The finding of the positive antipode to the phenomena occurring in anesthesia seems to us to give further validity to the principle that the suggestion of a rather simple sensory anomaly comes to involve, by a process within the organization of the human mind itself, rather far-reaching action tendencies.

The last phase of experimentation with subject N. was as follows: In a fully normal condition we proceeded to build up a conditioned response of withdrawal from the electrical contact to light stimulation. This conditioned response, was fully established after the first series of trials of 100. We carried, however, the subject through 300 more trials. Then we brought him into a post-hypnotic condition in which he was blind for the lamp. In 100 trials, now made, there was not a single withdrawal on light without shock, that is the tendency of the conditioned response was entirely inhibited. We then hypnotized the subject again, obtained rapport with an integrate of personality who admitted the inhibition, and gave to this integrate the contra-suggestion not to inhibit any impulses to withdraw during the experiment. Then we woke him up again in the post-hypnotic condition when he was blind to the lamp and continued the experiment. We had expected that he would now claim that he did not see the light and still would withdraw his finger from the contact every time the lamp was lighted, even when no shock accompanied this. What happened was however this: The subject kept raising his finger and put it back on the contact continually without any relation whatsoever either to shock or lighting of the lamp. This behavior continued as a kind of tic long after we had stopped both giving the shock and flashing the light. We thus failed in our endeavor to split up the mechanism set up by the suggestion of anesthesia.

The response we had been studying, that is the withdrawal of the finger from the contact, is not a true reflex. It is, without very much doubt, a semi-voluntary response. Would the same modifications by positive and negative hallucinations that we have studied in connection with this response, have occurred if we had had to do with true reflexes? From all general knowledge we have of clinical observations of hysterical anesthesias we would judge not. As a small side-issue investigation bearing on this point we made a study in two of the subjects of the behavior of the pupillary reflex and the eye-turning response—the latter is semivoluntary—on the one hand during stimulation by light for which the subject was blinded by suggestion and on the other hand during the condition when the stimulation was only a positive hallucination. The result was as follows: Whenever there was blindness for the lamp there was no eye-turning response when it was lighted, while the pupillary reflex always took place. Whenever the lighting up of the lamp was a positive hallucination coinciding with some other stimulation in another sensory field, the eye-turning response towards the lamp took place but there was not the faintest indication of any pupillary reflex. Observations concerning these matters were made independently by three observers and they all agreed to the facts mentioned.

Mrs. Lundholm and I have continued the experiments during the fall of 1927, and during this year, on a new subject, Mr. S., a student of psychology at Harvard, who has kindly volunteered as subject. The difference in our experimentation with Mr. S. has been that we now started to condition strongly a withdrawal of the finger from the electrical contact on the flashing up of the lamp during normal state. The conditioned response was easily established but to strengthen it we carried the subject through numerous series of trials on several consecutive days. Then we produced blindness in post-hypnotic condition for the lamp, and immediately every withdrawal of the finger on light without shock completely disappeared. In conditioning the response we had this time kept fairly equal intervals between the stimulations, and we had noticed a tendency in the subject to make a beginning withdrawal on the expectation of stimulation; in other words a rhythmical factor had been involved in the conditioning. This tendency to withdraw on expectation disappeared entirely in the post-hypnotic condition, where there was blindness for the lamp, which seemed to us again to indicate perhaps even stronger than previous evidence that with the suggestion of the anesthesia there

is really set up a co-conscious inhibitory tendency, relative to the withdrawal. In inducing the blindness for the lamp we had an experience that may be of some theoretical significance. The subject was brought to sleep and the suggestion given to him, read as follows: "You remember the experiments we have just been doing?" (This question referred to the series of trials in normal condition, given immediately before the hypnotizing. The subject answered in affirmative.) "Do you remember the little lamp you saw flash up during the experiment?" (Yes.) "When I wake you up in a little while and we continue the experiment, the lamp will not be there. You will not see it any longer." After we had awakened the subject we lighted up the lamp repeatedly to assure ourselves that he did not see it. In doing so we noticed that there was no eye-turning response towards the lamp when it flashed up, yet the subject's behavior indicated definitely that some kind of experience coincided with the lighting up of the lamp. On our question what was the matter, the subject told us that *something had been happening, he could not tell us what and where, except that it seemed to him to be in his eyes. Something like a flash.* We hypnotized the subject again and repeated the previous suggestion, adding a few words to the effect that when the subject now woke up this flashing in his eyes would also have disappeared entirely. When we had awakened him and again flashed the lamp a number of times he told us that even now something had been happening, but this time it was absolutely impossible for him in spite of strong urging to find any words to describe what had happened. The subject seemed to have had an experience carrying a minimum of meaning. The question raised by the observations is this: Is the effect of suggestion of anesthesia essentially an elimination of factors, without which an experience loses its proper meaning? It seems to us as if, by our first suggestion, certain factors of meaning had been blocked off from the experience of the subject, and as if, by the second suggestion, still further factors constituing the meaning had been blocked off. If this question can be reasonably answered in the affirmative a second one comes up. Are the hysterical anesthesias of the same nature? We would be interested to hear men with extensive experience both of hypnosis and hysteria, like Professor W. McDougall and Dr. Morton Prince comment upon these problems. When we carried on the experiments in post-hypnotic condition the subject was anesthetic for the nonadequate stimulation only to the extent described. In spite of this the withdrawal on

light without shock entirely disappeared and there was not the vaguest tendency during the post-hypnotic state of a reconditioning of the withdrawal by this experience with a minimum of meaning. On a number of consecutive days we continued to experiment with the subject alternatingly in normal and in post-hypnotic condition as above. Whenever the subject was aware of the flashing up of the lamp the conditioned response to light without shock always occurred, whenever his awareness of the flashing up of the lamp was an experience with a minimum of meaning, every single withdrawal on light without shock disappeared.

During January of this year, we have tried various modifications of our experiment. More interesting than the rest seems to us to be the result of the last session. We induced a periodically re-occurring anesthesia for the shock in post-hypnotic condition. The suggestion given to the subject during hypnotic sleep was as follows: "You remember the experiment we have just been doing?" (This refers again to a series of experiments in normal condition immediately preceding the hypnotizing of the subject. The subject answered our question in affirmative.) "When you wake up again, in a little while, we will continue to experiment. You will then see the light flash up again at certain intervals, as before, but you will get a shock only every fourth time the light flashes up. The very first time you see the light flash you will get a shock, then there will be three times of flashing of the light without shock, then the fourth time the light flashes you will again get the shock, then there will be three times of flashing without shock, then again a flashing of the lamp with shock and so on. Did you fully understand what I told you?" (Yes) "Repeat it." (The subject repeated the suggestion correctly.)

Then we woke him up and the results we now obtained were as follows: (Plus indicates withdrawal, minus absence of withdrawal. All trials were stimulations with both light and shock): +———+———+———+———+———+———, and so on indefinitely, with perfect regularity. In devising this experiment we had conceived of the possibility of obtaining just the result we got, but we had also taken into consideration another possibility, namely, that the effect of the strong conditioning of withdrawal from the contact by light without shock, that had been established during the thousands of trials in normal condition, might be felt in the post-hypnotic condition, and, there might express itself in a tendency to withdraw also on the three inter-

mittent stimulations when the shock was not sensed by the subject. The reason why this alternative was excluded as the experiment actually evolved itself became evident when we questioned the subject, while he was still in post-hypnotic condition, concerning his experiences. The essence of what he told us was this: He had never seen the light flash up except when he felt a shock and he voiced spontaneously his surprise that in this last experiment the intervals between the occurrence of shock plus light had been so very much longer than during all previous experiments. This final outcome of our suggestion seems to us interesting from two aspects: (1) In spite of the fact that the suggestion given to the subject,—a suggestion that he fully understood while asleep,—most definitely implies that he is going to see the light at times when he does not get the shock, the following thing takes place when he comes into the post-hypnotic condition: The anesthesia for three shocks out of four, which undoubtedly is there, as the subject does not withdraw on these three shocks, has made this part of our suggestion completely noneffective, resulting in the fact that the subject in the post-hypnotic condition becomes blind for any flashing of the light that does not coincide with the instance of a felt shock. We venture to believe that this association of anaesthesia for the shock with blindness for the light-flash is a result of the extensive experimentation in normal condition. (2) The regularity of the sequence of pluses and minuses indicates that the flashings of the light, that have not been consciously perceived in the post-hypnotic condition, yet must have been experienced by some system of the subject's personality. These flashes must have been preceived and more than that, they must have been counted by a consciousness that was co-conscious to the post-hypnotic personality-system.

We now decided to make another attempt to break up the tie between the anesthesia for shock and the associated blindness for the light. We hypnotized the subject again, and repeated our previous suggestion, emphasizing, however, very strongly that the subject really will see the light flash up three times without getting the shock. After we have had the subject repeat the suggestion, showing that he fully understood it we woke him up, and continued to experiment in the post-hypnotic condition, just as above. Every stimulation was thus a stimulation with both shock and light. The result we now obtained was as follows: Every fourth instance of stimulation, beginning with the first there was a clear withdrawal of the finger from the contact. The three inter-

mittent instances of stimulation when the subject saw the light but was not aware of any shock (this is his experience according to what he tells us himself after the series is through) he sometimes withdrew his finger definitely, sometimes there was a half withdrawal, or a jerk of the finger, sometimes there was no withdrawal at all. During the whole period of this experiment the subject showed the most definite signs of discomfort. We feel inclined to interpret the subject's inconsistent reactions and also his discomfort as indicative of conflict, and we venture to suggest, tentatively, that this conflict is between a tendency to inhibit withdrawal, set up by the suggestion of anaesthesia for the shock and a tendency to withdraw on light without shock, built up to a high degree of strength by our extensive experimentation in normal condition. In the experiment preceding the last one, this conflict was solved by a spontaneously occurring dissociation, opposed to our suggestion, the blindness for the light-flashes; in the last experiment, this dissociation was prohibited by our emphasis on a crucial point of the suggestion, and thus the subject came to suffer conflict. May not the first experiment perhaps be a laboratory parallel to certain types of hysterical disorder with dissociation, while the latter may be a laboratory parallel (why not?) to neurasthenia? We apologize for the boldness of such a suggestion, but we feel compelled to make it.

Again we have had to interrupt the work, as the examination-period approaches for our subject. We expect to continue the experiments with new subjects as soon as such are available, and we have devised various further modifications of the conditioned-response method, that we expect will then give further data. To summarize the result already obtained the following may be said: Functional anesthesia, as induced by hypnosis, seems to be, not merely a passive dropping out of a sensation-pattern from consciousness, but a condition involving a considerable amount of co-conscious conation, indicative of considerable co-conscious effort. This conation, as displayed in our experiments, appears to be, essentially, a co-conscious striving to act as if a sensory disorder is there. This striving to act as if, has been thwarted experimentally during our laboratory work, and we have had an opportunity to observe, both successful overcoming of the thwarting tendencies, escape-means of avoiding them by spontaneous hysteria-like? dissociation, and conflict with neurasthenia-like? discomfort, due to incomplete success in overcoming the enemy tendency and failure to solve the conflict by dissociation.

2. Suggestion towards a theory of functional anesthesia.

Any student of functional anesthesias, confronted with the obscure inconsistencies and sometimes paradoxical expressions of these disorders has, undoubtedly, felt like a child in a deep forest out of which no route seems to lead. Seeking light the discouraged scholar turns naturally to Pierre Janet, the king among the now living observers of dissociation, and Janet offers him a theory the essence of which is, in short, the following: Functional anesthesias occur in persons of a specific make up, the hysterical, and is there determined by a lacking mental tension, or lacking capacity of keeping organized in one personal hierarchy all the functions of the normal mind. Due to this lowered mental tension certain functions like, for instance, the function of sensing from the right hand, may drop out of the personal consciousness. The scholar may be satisfied with this view of just a passive dropping out of a function, but, if he has been trained in the "hemicycle" of Hormic Psychology, he may intuitively feel that there must be a positive conation behind the anesthesia, and to determine this positive conation may become the "Golden Fleece" of his ambition, when he again sets out on the Argonautic Trail.

Above is related exactly the experiences that inspired us to the experimental study of functional anesthesias that has been reported.

Looking back on the experiments, at the stage to which they have now proceeded, we feel confident that by suggesting the various anesthesias we have caused more than a passive dropping out of a function from the personal consciousness,—our suggestion has definitely set up a positive tendency, that becomes co-conscious in the post-hypnotic condition, of acting as if there were the sensory disorder; this tendency expressing itself in a definite co-conscious inhibition of habitual reactions.

The discovery of this tendency did not, however, in itself lead us to the tentative speculation concerning the nature of functional anesthesias that is going to be submitted here. There came in addition another observation that is reported on page 346 in the discussion of the experiments. This observation seems to imply that with one of our subjects the effect of a suggestion of post-hypnotic blindness for a lamp, may essentially be that, by some or other principle, the suggestion deprives the experience of the lighting of the lamp of the factors that give this experience its full meaning even reduces it to an experience carrying a minimum of meaning.

As this occurred to us we naturally began to consider the nature of the meaning of perceptual thought.

Professor McDougall, in his discussion of meaning, brings out that the meaning of any object of thought, in its widest sense, is all we know of the object, including its relation to numerous other objects. But the meaning of an object, the moment we do think of it, is mostly very much more limited, and is determined by the striving at work when we think the object. It seems to me that Professor McDougall has brought conclusive evidence for the truth of this by the illustration he gives in his "Outline of Psychology". I quote from this work a few passages to bear on the matter:

"Now note, that when I perceive the orange, there is somehow operative in the act of perception not only my previous experience of oranges, constituting the act one of recognition, but also a vast amount of other experience, experience of material objects of many kinds—experiences of seeing, of handling, of tasting, of hearing, and so forth. All this mass of past experience contributes to the "meaning" of the object, or, more strictly, my "meaning" as I perceive the orange. . . .

"Of the total 'meaning,' the total knowledge which is operative in the mind as I perceive the orange, one part or another is apt to be more prominent and to play a larger part than others in the guidance of my behavior, according to my purpose at the moment my glance falls on it. If I am thirsty and seeking to allay my thirst, I seize the orange or, at lease, perceive it as an object that will satisfy that desire; I perceive it as a succulent cool fruit; that part of its total 'meaning' is most prominent. If I am looking for a missile to throw at an intruding burglar, I probably pass it by. But, if I am looking for one to throw playfully at a friend, I may seize it for that purpose; then its weight, softness, its property of going 'squash' on impact, are the prominent part of the 'meaning' I find in the visual impression."

So far, Professor McDougall. Now another consideration: Is it not so that any perceptual thinking of an object is determined by striving? Is it not so that as far as an impression awakens a striving, or as far as it sustains a striving, it activates perceptual thinking? Only such an impression makes us attend to it and makes us fully cognize the object. Other impressions, non-related to any striving, do not activate perceptual thinking, they leave us unaware or anyhow very vaguely aware of their occurrence; they furnish only a very indistinct background pattern of awareness,

in which each part carries a minimum of meaning. I think I would be able to quote Professor McDougall also here for support of this view.

Let us now return to the experiments.

I emphasize again that, by our suggestion of an anesthesia, we apparently set up a tendency to act as if there were a sensory disorder. This tendency, we assume, with Professor McDougall, generates energy from the instinct of submission. Let us for the sake of a theory consider this tendency to be the primary result of the suggestion. This tendency becomes co-conscious in the post-hypnotic condition, but it serves continually to inhibit in the post-hypnotic personality any striving in relation to impression by the non-adequate stimulus. Now, the post-hypnotic personality is non-aware of this inhibitory tendency, yet he is, by it, incapacitated of any striving relative to a certain impression. How would this situation influence his awareness of latter impression? Would it not, in accordance with our previous general discussion of striving and meaning, strip this impression of all the factors constituting its meaning and leave it an experience carrying a minimum of meaning? If the co-conscious inhibitory tendency is fully successful, would not then the impression be deprived of meaning to an extent to make its value in the sphere of personal consciousness equal null? Might not this be exactly the nature of the anesthesia our subject suffers?

The theory we thus suggest concerning the nature of functional anesthesia, as induced by hypnosis, may in short, be restated as follows: The suggestion of an anesthesia primarily builds up a tendency to act as if this defect were there. This tendency which becomes co-conscious to the post-hypnotic personality, inhibits in him any striving in relation to impressions received through a certain sensory area. This deprives such impressions of meaning to an extent to make the subject suffer an apparent anesthesia for them.

It seems to us as if this theory may be inversely applied to the explanation of positive hallucinations as induced by hypnosis. When we suggested to our subject, that any time he got a shock he would see a lamp light up, we probably set up in him a tendency to strive as if this lamp really flashed (one manifestation of this tendency was later the eye-turning response that mostly took place whenever the shock was applied). In the post-hypnotic condition this tendency became co-conscious, but it continued to be activated every time the shock was given. This striving determined cogni-

tion relative to the lamp, and added to the impression of the non-lighted lamp such factors as to induce in the subject the meaning of "lamp lighting up"—a beautiful illustration, if our interpretation is true, of the principle that conation may determine cognition.

There is one feature of the experiments that apparently seems to go against the theory now offered In two of our subjects we found an outlasting into a fully normal condition of the inhibitory impulse relative to withdrawal on sound without shock, that had been previously strongly established during extensive experimentation in a post-hypnotic condition when the subject suffered artificially induced functional deafness. If this inhibitory impulse continued to act co-consciously and prevent the building up of the conditioned response in the normal condition, why did it not produce deafness in the normal condition or in other words, why did it not throw the subject back into the condition of the post-hypnotic period. We feel unable to answer satisfactorily this question, and humbly admit that it is a blow to the theory. It may however be that the existence of this co-conscious inhibitory tendency effected some modification of meaning of the subject's sound impression in the normal condition; not enough to extinguish it entirely as sound, but enough perhaps to prevent the subject from clearly cognizing the coincidence of sound and shock. This is, however, only a guess on loose grounds, and, we admit it, a poor contra-argument in favor of the theory. If we had known what the further experimentation would lead us to believe, we would have investigated the nature of our subjects cognition relative to his sound impression during the period of experimentation in "normal" condition. Here is a problem of further research if we would again, in a new subject, meet this outlasting of the inhibitory impulse during such condition. We do not feel that the contra-indication relative to the truth of our theory, that is discussed in this paragraph, necessarily compels us to give up the theory but it compels us to emphasize again its tentative nature.

And now a few words about the spontaneously occurring anesthesias in hysteria, in the light of our theory.

May it be possible that also the latter anesthesias are determined by analogous principle as the one we have suggested relative to anesthesias, artificially induced by hypnosis? We are inclined to think: Yes. The main difference would only be this, that in hysterical anesthesia, the co-conscious inhibitory tendency which prevents the conscious personality to strive in relation to

certain impressions, does not generate, necessarily, energy from the submissive instinct, but from some or other different wish, as for an instance, a wish to escape, or a wish to obtain sympathy and through it any other desired goal.

If our theory concerning functional anesthesia is true, it opens a relation between the fairly simple phenomena of functional insensibilities and very complex psychical obsession. To prepare for the understanding of this, permit me to tell a story, or rather to relate a memory.

I used to have a friend who was an autodidact. Independent of any guidance by high school or college, he had acquired a considerable amount of information concerning matters of philosophy and the psychology of religion. He and I frequently met in his studio, and, while the crystalline twilight of the Nordic summer eve slowly fell, we used to discuss various questions of psychology. As I retrospect now on our sessions, I can recall how, on certain occasions when he was in a particular state of mind I used to be annoyed by the obstinacy with which he seemed to oppose any suggestion attempted, whether it came from me or from somebody else. It seemed to me as if any positive statement of mine, concerning whatsoever, immediately set up in him an ambi-tendency to think differently. I sometimes pointed out to him this oppositional obstinacy of his before we separated after a hot discussion. On such occasions he seemed to be only vaguely aware of it himself, and only when I reviewed the course of our conversation did he grant I was right, and seemed to be a little ashamed. Another thing was also striking: Whenever he was in the ambi-tendencial state of mind he lost considerably his capacity to grasp an argument, this to a point that he sometimes thought that he was defeating me while he in fact expressed a view that I had just before approved of. When I interrupted to tell him that, what he had been arguing for, during the last five minutes was exactly what I thought myself and had expressed previously, he showed signs of embarrassment or surprise.

May it be that the principle underlying this singular tendency of my friend was the following: He was a man who, without having been favored by sympathetic guidance, since boyhood up had sublimated a strong inferiority-sentiment in striving for intellectual acquisitions. This is so far only sound sublimation, but, as time goes on, the inferiority-sentiment begins to generate energy into the less sound compensatory channels of superficial assertion and pride. And now the inferiority-sentiment, which

becomes less and less conscious, has set up a tendency that is detrimental to continued intellectual development, a tendency that is essentially inhibitory to further acceptance from others, even to curiosity concerning other people's views. This tendency becomes activated by any positive intellectual impression from others, drains into other channels the impulse that should go into curiosity and attention relative to the impression, and thus impairs the capacity to understand the full meaning of an argument.

If this rapid sketch of the psychogenesis of my friend's obsessional ambi-tendency on the intellectual plane is correct, is not then his case in a way analogous to functional anesthesia? Have we not here, in a less extreme degree, a modification, an enpoverment, of the meaning of objects of thought by an assumably co-conscious, or, anyhow, far from conscious tendency that is inhibitory of proper striving relative to certain social impressions.

My friend's case can, without doubt, be stated as a case of prejudice, and, as the principle underlying functional anesthesia, in a wide sense, can be applied to it, this principle can, I think, in the same sense be applied to any case of prejudice. (Note the expression: Blind prejudice.) This extends again the domain of the principle through a range of increasingly maladjusted prejudices unto the delusions of certain insane, such as the paranoid and the schizophrene. In these extreme instances, just as in any case of prejudice, there are probably co-conscious tendencies, generating energy from an all-powerful egotistic wish, which modify normal conscious striving, thus changing the meaning of things. In the schizophrene the impressions of the objective world in toto becomes, by such principle, deprived of the factors of meaning, constituting its reality-criterion, to an extent that the line of demarcation between reality and fantasy extensively dissolves. There occurs, as a result of the activity of such co-conscious tendencies (why not?) an anesthesia, a blindness for the reality criterion, or in other words, the function of discriminating between reality and fantasy becomes dissociated.

Waverly, February 9, 1928.